for a short period in the dark room, on developing the latter a reversed negative (or *phosphogram* as I prefer to call it) of the original picture is permanently produced. If the picture consists (as in my case) of a vista of smoke-stained chimney-pots, the slated roofs and stuccoed walls of adjacent houses, the negative, under favourable circumstances, will show considerable traces of natural colours. In the case of phosphorescent calcium sulphide, which glows with a beautiful purple violet light, it is not difficult to understand why it acts on the gelatine film so readily as it does. Both the phosphorescent rays and the most active photo-chemical rays belong (as is well known) to the most refrangible rays of the solar spectrum; that the one should thus react on the other is not surprising.

If, instead of using the colourless calcium sulphide, as above described, we stain the mixture with an alcoholic solution of rosanilin acetate before applying it to the glass plate, on exposure to daylight it glows with a deeper intensity, and it still acts, although with less vigour, on a photographic film. M. Verneuil (*Comptes rendus*, civ. 501), and M. Becquerel (*ibid.*, 551), have investigated the causes of phosphorescence in calcium sulphide from a chemical standpoint, and the latter has shown that the colour of the light may be changed to a bright green by the admixture of a trace of lithium carbonate or of potassium persulphide, and to an orange-yellow by the addition of manganese peroxide. The green phosphorescence acts feebly, the orange-yellow scarcely at all, upon the ordinary gelatine film. The bromo-iodide plates are clearly only imperfectly responsive to coloured rays. To attempt coloured photography with the present dry plates is eminently unsatisfactory. By staining the film some makers have succeeded in increasing the sensitiveness of the plate to the less refrangible red rays, but my own experience leads me to conjecture that the green and green-yellow rays are quite as difficult to intercept by our modern plates as the deep-red, if they are not more so.

Brighton, March 28. W. Ainslie Hollis.

Neo-Lamarckism and Darwinism.

Under the above heading (Nature, March 26, p. 490) Prof. Henslow gives some criticisms of my paper on the Alpine flora, which seem to show that, for the sake of brevity, I unintentionally rendered my views obscure to him and possibly to others who share his opinions. It is therefore fortunate that he should have stated his difficulties, in order that I may try to explain what it was I intended, taking the various points in succession.

(1) I wrote "dwarfing may be—doubtless often is—the direct result of environment, as lack of nourishment." This, I believe, is strictly true as regards individual plants, and I do not think it affects the argument that dwarfing may also be due to other causes. Certainly, insufficient nutriment may cause extreme dwarfing altogether apart from cold: for instance, I very well remember some minute but flowering specimens of *Matricaria*, only an inch or so high, found by Mr. J. W. Horsley in a dry place near Chiswick, where the normal fully-developed form of the plant is abundant.

And if dwarfing is the result of cold, how is it explained that plants on different soils at the same altitude vary so much in this respect? Thus, along Swift Creek, in Colorado, *Mertensia sibirica*, which grows in very damp ground and in the rich vegetable deposits on the immediate banks of the creek, is large, tall, and rank, with broad leaves; while a species of the same genus, growing close by on the more exposed, dry, and more barren prairie land, is very much smaller, more compact, and with narrow leaves. Again, at the same place, *Oxytropis lamberti*, when growing in the damper ground by the creek, is tall, rank, and has white or very pale flowers; while on the adjoining prairie the same species is low, depressed, altogether smaller, and with crimson flowers, which turn purple in drying for the herbarium.

I did not say, and certainly would not say, that dwarfing was always the result of bad or insufficient nourishment; but neither is it always or normally the direct result of cold.

(2) When dwarfed varieties or forms have been produced by environmental conditions or otherwise, it is natural enough that they should tend to revert to the original type when removed again to the original conditions. Nevertheless, true Alpine dwarfed *species* do retain their characters when grown at low altitudes artificially. Florists' catalogues are full of allusions to dwarf varieties and species, most of which are very constant. As I write, I can see a clump of *Scilla sibirica* out of the window, and certainly it appears to me to have the true characters of the species, and the long cultivation which this species has undergone has not changed it into a large and more diffuse species like *Scilla nutans.* Minute species of *Narcissus*, *Gentiana*, and *Silene*, which come from mountain regions, may surely be grown in England without starting up and simulating the large temperate-region forms of those genera. I don't mean to say that dwarfing, like other specific characters, may not become changed or lost in time if the circumstances are favourable, or the utility of the peculiarity ceases, but I think it will be generally admitted that low stature, as a specific character, often proves as constant and hereditary as other specific characters.

(3) Surely it is not necessary to demonstrate that tall plants would be likely to be injured by the winds on Alpine summits? The trees at timber-line in Colorado would, I should think, convince anybody of this. Those which are exposed frequently bear branches only on the side towards the valley, the others having succumbed at an early stage to the violence of the winds. The trees at their highest limit are shorn off almost as if with a knife, so that, going down the slope, one does not meet with a full-sized tree until the topmost branches are able to obtain some shelter from the tree above it; so that, although the trees grow to timber-line, they do not raise their summits above it. Also, one may find some old stumps, perhaps fifty yards above the present timber-line, on the Sangre de Cristo Range, showing that, for some reason, trees which formerly grew there have since succumbed.

(4) I have not myself made any experiments to test the additional warmth that might be derived from close proximity to the ground, but I think the point is well established. The rocks and the surface of the ground would surely retain a certain amount of heat, and beds of vegetable tissue, such as peat, do not readily cool. Thus, Mr. E. J. Lowe relates (*Conchologist*, 1891, p. 4) that, in the great frost of 1860-61, the River Trent was frozen over; but a drain, cut through a bed of organic soil, though only a foot wide, and containing less than 12 inches in depth of water, remained unfrozen.

(5) As to "partial shelter": is it not clear that dwarfed plants, which grow close to the ground, and often between rocks or under projecting ledges (as in the manner of Alpine plants), will be partially sheltered, and thereby advantaged?

(6) Of course, I do not suppose that plants *know* anything about the coming season! Only this, those plants which naturally grew quickly and through rapid metabolism came to early maturity, would survive, while those which did not do so would perish. It is a simple example of natural selection. Various weeds and other wild plants, notably *Cleome integrifolia*, are in Colorado often accidentally sown as seeds dropped from hay, and thus appear round human habitations some hundreds of feet above their proper altitude. They grow and flourish, and duly flower, but they cannot mature their seeds in time, and hence never perpetuate themselves at these altitudes. The Indian corn, *Zea maïs*, varies considerably in its period for maturing. It can be grown in England, but does not sufficiently often mature to be a paying crop. In America, the quickly-maturing varieties can be grown at greater altitudes and latitudes than the others. Given such variation, is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short? Let anyone take varieties of Indian corn differing in the period of maturing, and grow them at the extreme altitude at which this species can be cultivated, and it will not take a botanist to predict the success of the one and the failure of the other.

T. D. A. Cockerell.

The Whirling Ring and Disk.

I regret that the interjection of a paradoxical rider to my proposition regarding a whirling ring should have had the effect of embedding in a storm of protest Prof. Ewing's interesting point concerning a disk. Let me do the best I can to set matters straight and clear.

First, it is plain that my rider concerning a cable was not well worded, and whereas it is quite true that a weightless steel girdle round the earth would be broken by its whirling tension, it would have been better to avoid any appeal to flotation as a practical means of securing weightlessness. The simplest plan is to abandon the cable illustration alto-

© 1891 Nature Publishing Group

gether as a not very pertinent or useful one. Nevertheless, it is plain that a cable fixed at two distant points and hanging free between them is liable to break, whether it be hanging downwards or floated upwards, *i.e.* whether it be supported too little or too much; and it further appears that if it be stretched to the curvature of the earth the slight difference between its true and apparent weights, *i.e.* its centrifugal force alone, is sufficient to break it.

But now returning to the main proposition, that the critical velocity at which a whirling ring breaks is the square root of the ratio of its tenacity to its density, no one calls it in question; some, indeed, say it is well known. That it was known, or at least so easily knowable as to be practically known, to applied mathematicians, is manifest; but that it is well known to practical men I have some reason for doubting. However that is of no consequence. What I write to point out is how singularly nearly the result for a ring agrees with the result for a disk of the same size.

For, as Prof. Ewing's letter shows, the critical peripheral speed of a uniform disk with a very small hole in the centre, when it is on the point of flying, is

$$\sqrt{\left(\frac{4\mathrm{T}}{(3+\mu)\rho}\right)},$$

whereas for a ring it is the same expression without the $4/3\frac{1}{4}$.

Thus, then, a perforated disk of uniform thickness, although its greatest stress occurs round its central aperture, yet can stand only 11 per cent. greater angular spin than a ring of the same material fitting its circumference and devoid of all radial support.

A disk without a hole can, as Prof. Ewing says, stand 40 per cent. more rotations per minute than a perforated disk can stand (*i.e.* $\sqrt{2}$ times the speed), even though its perforation is a mere needle-prick or indeed is absolutely infinitesimal, so long as it destroys radial coherence across the centre.

I learn also from Prof. Ewing that Mr. Chree has obtained results for an oblate spheroid, which, reduced to an approximate disk, show that the stresses and strains at any point of the ellipsoid are $\frac{8}{11+\mu}$ths of what they are at the corresponding point of an unperforate disk of the same size but of uniform thickness (μ being Poisson's ratio).

I am obliged to Mr. Boys for calling attention to Maxwell's early memoir on the subject ("Scientific Papers," vol. i. p. 61), with which I was not acquainted. Since Maxwell uses an unusual notation, it may be convenient to note that his μ is volume-elasticity, his m is twice rigidity, and E = Young's modulus; so that his E/m is merely $1 +$ Poisson's ratio. There are, however, a few misprints with regard to signs.

Prof. Worthington's remarks refer to a straight *bar* with free ends, not to an endless band. It is true, however, that there was no need to drag in vibrations; the dangerous tension will be set up in a straight portion of any endless band, running in the direction of its length with the critical speed $\sqrt{\left(\frac{\mathrm{T}}{\rho}\right)}$, by the agency of the curved portions, which necessarily exist somewhere.

It may save some confusion to mention that the suffixes of Prof. Ewing's radial and tangential tensions have become interchanged in the first column of his letter on p. 462 (March 19).

The observations of Prof. Greenhill are best dealt with under a separate heading.

Oliver J. Lodge.

March 21.

Formation of Language.

Permit me to reply to your correspondent Mr. W. J. Stillman, on the "Formation of Language" (Nature, March 26, p. 491). The interesting fact he records of the spontaneous invention and use of child-names for objects is not unknown to philologists. The phenomenon has been previously noticed, among others, by Miss Watson, of Boston, and Dr. E. R. Hun, of Albany, U.S.A.; by Archdeacon Farrar, in the case of Indian children left by themselves for days together in Canadian villages; and by M. Taine, in his work "De l'Intelligence." Numerous examples of children's language are given by Dr. Horatio Hale (philologist to the U.S. Wilkes Exploring Expedition), who has made a special study of the spontaneous development of roots among children the basis of his remarkable theory of the origin of linguistic stocks. Full details will be found in a paper on the "Development of Language," read before the Canadian Institute of Toronto, April 1888, and in an address on the "Origin of Languages and the Antiquity of Speaking Man," in the Proceedings of the American Association for the Advancement of Science, Buffalo, 1886, vol. xxxv. The occurrence has been often noticed in the families of philologists—the most noteworthy instance being that of the young nephew of the well-known Sinologist Dr. George von Gablentz. This boy, before he learned his mother-tongue, called things by names of his own invention. The constant elements were the consonants, the vowels being varied, and employed as they were deeper or higher to denote greatness or smallness. The root for round objects was *m-m*; a watch, plate, and the moon was *mem*, a large round dish or table, *mum*, and the stars *mim mim mim*; an ordinary chair was *lakail*, a great arm-chair *lukull*, and a little doll's chair *likill.* A distinguished Accadian, Chinese, and Semitic scholar, the Rev. C. J. Ball, makes no secret of the fact that, between the ages of six and eight, he and his young brother had names of their own devising—perfectly arbitrary monosyllables and dissyllables for several of the small tools and toys they valued most. Mary Howitt relates in her autobiography (edited by her daughters) that the silent sadness of the Quaker home circle extended to the nursemaids, and that in consequence of this the eldest child, her sister Anna, did not learn to talk until she was four years old. Long after they could talk, "being left chiefly to converse together, our ignorance of the true appellations for many ordinary sentiments and actions compelled us to coin and use words of our own invention. To sneeze was to us both *okis-kow*, the sound which one of our parents must have made in sneezing." Here we get a true *onomatopœia*, as in the monosyllable *mea*, employed by one American child for "cat"; in another child's vocabulary the extraordinary trisyllable *shindikik* designated that animal. The association of ideas and extension of meaning are often very suggestive—viz. *migno migno* = water, wash, bath; *waia waiar* = black, darkness, Negro. As in the case of the name for water, *bhumboo*, cited by Mr. Stillman, the vocables are often of two syllables, rarely of three. It is interesting to note the continued use of the little boy's own name for water as a means of identifying the acquired Italian *aqua* for the same object, as frequently happens with adults struggling to express themselves in a foreign tongue. Reduplication seems also to characterize these "child languages" like those of some savage tribes, and plurals are formed by repetition. The syntax, Dr. Hale remarks, resembles that of deaf-mutes and gesture language. If left to themselves there seems no reason why children with this aptitude should not develop a vocabulary at least as extensive as that of Dr. Farrar's three peasants, "who conversed for a long while without employing more than one hundred words." Many cases of "child language," no doubt, have passed away unrecorded. Soon after the children mix much with adults, the special vocabulary begins to die out. It is possible that the use of such spontaneously developed root-words might be prolonged among the children of the poorer classes, so often cared for by children but little older than themselves. The *crèches* of our large towns might afford further evidence of abnormal developments of this apparently inherent inventive linguistic faculty.

Brighton, April 2.

Agnes Crane.

Mr. W. J. Stillman winds up his interesting letter on the above subject by inquiring whether any of the readers of Nature have made observations analogous to his own on the development of language in children.

I have on several occasions requested parents to note down the sounds emitted by their children from their birth to the time when those sounds seemed to become developed into articulate words with a certain amount of intelligent meaning, but I have not obtained any satisfactory results. The subject has, however, been studied, and described with considerable detail, by that well-known member of the French Academy, M. H. Taine, in his work, "De l'Intelligence" (2 vols., 3rd edition, Paris, 1878). I would particularly refer to chap. ii., sec. 5, and also to that part of a long note at the end of vol. i., entitled "Acquisition du Langage par les Enfants" (pp. 356–383).

C. Tomlinson.

Highgate, N., March 31.

© 1891 Nature Publishing Group